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# SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE  
OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION  
FOR THE ADVANCEMENT OF SCIENCE.

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## ADDRESS OF THE PRESIDENT TO THE MATHEMATICAL AND PHYSICAL SECTION OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE<sup>1</sup>

It is with much misgiving that I endeavor to discharge the traditional duty of the president of a section of the British Association. So many other duties seem to find a natural resting-place with any one who has to reckon at the same time with the immediate requirements of the public, the claims of scientific opinion, and the interests of posterity, that, unless you are content with such contribution towards the advancement of the sciences of mathematics and physics as my daily experience enables me to offer you, I shall find the task impossible.

With a leaning towards periodicity perhaps slightly unorthodox I have looked back to see what they were doing in Section A fifty years ago. Richard Owen was president of the association, William Whewell was president of Section A for the fifth time.

At the meeting of 1858 they must have spent some time over nineteen very substantial reports on researches in science, which included a large section of Mallett's facts and theory of earthquake phenomena, magnetic surveys of Great Britain and of Ireland, and, oddly enough, an account of the self-recording anemometer by Beckley; perhaps a longer time was required for fifty-seven papers contributed to the section, but very little was spent

<sup>1</sup> Dublin, 1908.

over the presidential address, for it only occupies two pages of print. My inclination towards periodicities and another consideration leads me to regard the precedent as a good one. That other consideration is that Section A has always more subjects for discussion than it can properly dispose of; and, in this case, discipline, like charity, might begin at home.

Since the section met last year it has lost its most illustrious member and its most faithful friend. Lord Kelvin made his first contribution to Section A at Cambridge in 1845, on the elementary laws of statical electricity; he was president of the section in 1852 at Belfast for the first of five times. I have looked to see what suggestion I could derive from his first essay in that capacity. I can find no reference to any address in the published volume. I wish I had the courage to follow that great example.

Lord Kelvin's association with Section A was so constant and so intimate that it requires more than a passing word of reference. There is probably no student of mathematics or physics grown into a position of responsibility in this country but keeps among his treasured reminiscences some words of inspiration and of encouragement from Kelvin, spoken in the surroundings which we are once more met to inaugurate. I refer to those unrecorded acts of kindness and help because they were really a striking characteristic of Section A. Their value for the amenity as well as for the advancement of science it would be difficult to over-estimate. I could not, even if time permitted, hope to set before you an adequate appreciation of Kelvin's contributions to science as illustrated by his communications to this section, and in this place it is not necessary. But I can not pass over that feature of his character without notice.

Closely following on the loss of Kelvin

came the death of Sir Richard Strachey, a personal loss to which it is difficult to give expression. I am not aware that he had much to do with Section A. I wish, indeed, that the section had seen its way to bring him more closely into touch with its proceedings. He was president of Section E in 1875, and, by appointment of the Royal Society, he was for twenty-two years chairman of the meteorological council. I had the good fortune to be very closely associated with him during the last ten years of his life, and to realize the ideas which lay behind his official actions and to appreciate the reality of his services to science in the past and for the future.

These losses unfortunately do not stand alone. Only last year Sir John Eliot received the congratulations of all his fellow-workers upon the publication of his "*Climatological Atlas of India*" as representing the most conspicuous achievement of orderly, deliberate, purposeful compilation of meteorological facts for a special area that has yet been seen. He was full of projects for a handbook to accompany the atlas, and of ideas for the prosecution of meteorological research over wide areas by collecting information from all the world and enlisting the active cooperation of the constituent parts of the British empire in using those observations for the advancement of science and the benefit of mankind. He died quite suddenly on March 18, not young as years go, but quite youthful in the deliberate purpose of manifold scientific activities and in his irrepressible faith in the future of the science which he has adorned.

The section will, I hope, forgive me if I put before them some considerations which the careers of these three men suggest. Kelvin, a mathematician, a natural philosopher, a university professor, some part of whose scientific work is known to each one of us. He was possessed with the notion that

mathematics and natural philosophy are applicable in every part of the work of daily life, and made good the contention by presenting to the world, besides innumerable theoretical papers, instruments of all degrees of complexity, from the harmonic analyzer to an improved water-tap. It was he who transfigured and transformed the mariner's compass and the lead-line into instruments which have been of the greatest practical service. It was he who, when experimental science was merely a collection of facts or generalizations, conceived the idea of transfiguring every branch of it by the application of the principles of natural philosophy, as Newton had transfigured astronomy. The ambition of Thomson and Tait's "Natural Philosophy," of which only the first volume reached the stage of publication, is a fair index of Kelvin's genius.

Strachey, on the other hand, by profession a military engineer, a great administrator, head of the Public Works Department in India, deeply versed in finance and in all the other constituent parts of administration, by his own natural instinct demanded the assistance of science for every branch of administration. In promoting the development of botany, of meteorology, or geodesy and of mathematics, he was not administering the patronage of a Macænas, but claiming the practical service of science in forestry, in agriculture, in famine relief, in public works, and in finance. You can not gauge Strachey's services to science by the papers which he contributed to scientific societies, if you leave out of account the fact that they were really incidents in the opening of fresh channels of communication between scientific work and the public service.

And Eliot, as meteorological reporter to the government of India, an accomplished mathematician (for he was second wrangler

and first Smith's prizeman in 1869), a capable and devoted public servant, the medium by which Strachey's ideas as regards the use of meteorology in administration found expression in the government of India, who caught the true perception of the place of science in the service of the state, and made his office the indispensable handmaid of the Indian administration. These three men together, who have all passed away within a space of three months, are such representative types of scientific workers, complementary and supplementary, that a similar combination is not likely to occur again. All three indispensable, yet not two alike, except in their enthusiasm for the sciences for the advancement of which Section A exists.

To these I might indeed add another type, the private contributor to the physical exploration of the visible universe, of which Ireland furnishes so many noble examples; and in that connection let me give expression to the sense of grievous loss, to this association and to science, occasioned by the premature death of W. E. Wilson, of Daramona, a splendid example of that type.

In the division of the work of advancing the science of mathematics and physics and their application to the service of mankind, I am reminded of Dryden's somewhat lopsided comparison of the relative influence of music and song in his *Ode to St. Cecilia's Day*. If I may be pardoned for comparing small things with great, the power of Timotheus's music over Alexander's moods was hardly less complete than Kelvin's power to touch every department of the working world with his genius. But I may remind you that, after a prolonged description of the tremendous influence of Timotheus upon the victorious hero, the poet deals in one stanza with his nominal subject:

At last divine Cecilia came,  
 Inventress of the vocal frame;  
 The sweet enthusiast, from her sacred store,  
 Enlarged the former narrow bounds

With nature's mother-wit, and arts unknown  
 before.

Let old Timotheus yield the prize,  
 Or both divide the crown;  
 He raised a mortal to the skies,  
 She drew an angel down.

I doubt if any of my hearers who knew Strachey by sight would recognize in him the scientific reincarnation of St. Cecilia, but it is none the less true that he was pre-eminent among men in inventing the means of drawing angels down and using their service for the attuning of common life to a scientific standard. It may be equally hard for those who knew him to look upon Eliot as a vocal frame, for of all his physical capacities his voice was the least impressive; and yet it is not untrue to say that he was conspicuously a medium by which the celestial harmonies of the physical sciences were brought into touch with the practical life of India through his work, which is represented by a considerable number of the twenty volumes of *Memoirs* of the Indian Meteorological Service.

I do not indulge in this poetic extravagance without some underlying reason. Speaking for the physics of the atmosphere, there is a real distinction between these three sides of scientific work. To some is given the power of the mathematician or the physicist to raise the mortal to the skies, to solve some problem which, if not in itself a meteorological one, still has a bearing, sooner or later to be discovered and developed, upon the working of atmospheric phenomena. It is easy enough to cite illustrious examples: among notable instances there recur to my mind Rayleigh's work on the color of the sky and Pernter's meteorological optics; papers

by Ferrel and others on the general circulation of the atmosphere; Kelvin and Rayleigh on the elastic oscillations of the atmosphere; the papers by Hagen, Helmholtz, Oberbeck, Margules, Hertz and Von Bezold on the dynamics and thermodynamics of the atmosphere, collected and translated by Cleveland Abbe; the work on atmospheric absorption by Langley and the theoretical papers on radiation by Poynting; those on condensation nuclei by Aitken and Wilson, and the recent work on atmospheric electricity, including the remarkable paper by Wilson on the quiet transference of electricity from the air to the ground.

But these things are not of themselves applied to the meteorology of every-day life. It is, in a way, a separate sense, given to few, to realize the possibilities that may result from the solution of new theoretical problems, from the invention of new methods—to grasp, in fact, the idea of bringing the angels down. And, in order that the regular workers in such matters may be in a position constantly to reap the advantages which men of genius provide, the vocal frame must have its permanent embodiment. For the advancement of science in this sense we require all three—the professor with academic freedom to illuminate with his genius any phenomenon which he may be pleased to investigate, the administrator, face to face with the practical problems in which science can help, and the living voice which can tune itself in harmony with the advances of science and in sympathy with the needs of the people whom it serves.

The true relations of these matters are not always apparent. Eliot, bringing to the work of the Indian Meteorological Office a mind trained in the mathematical school of which Kelvin was a most conspicuous exponent, achieved a remarkable

success, with which perhaps my hearers are not familiar.

In this country there is a widespread idea that meteorology achieves its object if by its means the daily papers can give such trustworthy advice as will enable a cautious man to decide whether to take out his walking-stick or his umbrella. Some of us are accustomed to look upon India as a place of unusual scientific enlightenment, where governments have a worthy appreciation of the claims of science for recognition and support. But Eliot was never tired of telling me that it was the administration of India, and not the advancement of science, that the Indian administrators had in view; and among his achievements the one of which he was most proud was that the conduct of his office upon scientific lines during his tenure had so commended itself to the administrators that his successor was to be allowed three assistants, with special scientific training, in order that the state might have the benefit of their knowledge.

It is, of course, easy to suggest in explanation of this success that the Department of Public Works of India can not afford to be unmindful of the distribution of rainfall, and that there is an obvious connection between Indian finances and Indian droughts; but it is a new fact in British history that the application of scientific considerations to the phenomena of rainfall are of such direct practical importance that meteorological information is a matter of consequence to all government officials, and that meteorological prospects are a factor of finance. Imagine his Majesty's Chancellor of the Exchequer calling at 63 Victoria Street to make inquiries with a view to framing his next budget, or taking his prospects of a realized surplus from the "Daily Weather Report." Yet in India meteorology is to such an extent a

public servant that such proceedings would not excite remark.

To have placed a scientific service on such a footing is, indeed, a notable success. Again, I rely upon Eliot when I say that that success is only to be achieved by being constantly on the watch to render service wherever service can be rendered. There is a difference between this attitude and that which has for its object the contribution of an effective paper to a scientific publication; in other words, it must be frankly recognized that the business of the scientific departments of government is not to raise an occasional mortal to the skies, but to draw down as many angels as are within reach. I was much surprised, when Eliot wished to develop a large scheme for meteorological work on a wider scale, that he made his appeal to the British Association as chairman of the Subsection for Cosmical Physics at Cambridge, and thereby to the governments of this country and the colonies. He felt that he could only urge the Indian government to join, and he did so successfully, so far as India would be directly benefited thereby, however important the results might be from a purely scientific point of view. Strange as it may appear to some, it was to this country that he looked for assistance, on the plea of the increase of knowledge for its own sake, or for the sake of mankind at large.

I am disposed, therefore, to carry your thoughts a little further, and rely on your patience while I consider another aspect for the process of drawing down the angels from the mathematical and physical sky, a process which is sufficiently indicative of the functions of a state scientific department. Viewing the world at large, and not merely that part of it with which we are ourselves immediately concerned, such departments deal with celestial physics in astronomy, with the physics of the air in meteorology and atmospheric electricity,

with the physics of land and water in physical geography and geology, seismology and terrestrial magnetism, oceanography and hydrography. It is for the practical applications of these sciences to the service of the navigator, the fisherman, the husbandman, the miner, the medical man, the engineer and the general public that there is an obvious public want.

Let me carry you with me in regarding these departments, primarily, as centers for establishing the growth of science by bringing it to bear upon the practical business of life, by a process of regular plantation, and not the occasional importation of an exotic scientific expert. I shall carry you with me also if I say that the gravest danger to such scientific institutions is the tendency to waste. I use the term "waste" not in its narrowest, but in its most liberal sense, to include waste of money, waste of effort, waste of scientific opportunity. I do not regard it as a waste that such a department should be unable to emulate Timotheus's efforts. Any aspiration in that direction is, of course, worthy of every encouragement, but the environment is not generally suitable for such achievements. I do, however, regard it as waste if the divine Cecilia is not properly honored, and if advantage is not taken of the fullest and freest use of the newest and best scientific methods, and their application in the widest manner possible.

I speak for the office with which I am connected when I say its temptations to waste are very numerous and very serious. It is wasteful to collect observations which will never be used; it is equally wasteful to decline to collect observations which in the future may prove to be of vital importance. It is wasteful to discuss observations that are made with inadequate appliances; it is equally wasteful to allow observations to accumulate in useless heaps because you are not sure that the instru-

ments are good enough. It is wasteful to use antiquated methods of computation or discussion; it is equally wasteful to use all the time in making trial of new methods. It is wasteful to make use of researches if they are inaccurate; it is equally wasteful to neglect the results of researches because you have not made up your mind whether they are accurate or not. It is wasteful to work with an inadequate system in such matters as synoptic meteorology; it is equally wasteful to lose heart because you can not get all the facilities which you feel the occasion demands.

It is the business of those responsible for the administration of such an office to keep a nice balance of adjustment between the different sides of activity, so that in the long run the waste is reduced to a minimum. There must in any case be a good deal of routine work which is drudgery; and if one is to look at all beyond the public requirements and public appreciation of the immediate present, there must be a certain amount of enterprise and consequently a certain amount of speculation.

Let me remark by the way that there is a tendency among some of my meteorological friends to consider that a meteorological establishment can be regarded as alive, and even in good health, if it keeps up its regular output of observations in proper order and up to date, and that initiative in discussing the observations is exclusively the duty of a central office. That is a view that I should like to see changed. I do not wish to sacrifice my own privilege of initiative in meteorological speculation, but I have no wish for a monopoly. To me, I confess, the speculation which may be dignified by the name of meteorological research is the part of the office work which makes the drudgery of routine tolerable. For my part, I should like every worker in the office, no matter how humble his position may be, somehow or other to have the opportunity

of realizing that he is taking part in the unravelling of the mysteries of the weather; and I do not think that any establishment, or section of an establishment, that depends upon science can be regarded as really alive unless it feels itself in active touch with that speculation which results in the advancement of knowledge. I do not hesitate to apply to other meteorological establishments, and indeed to all scientific institutions that claim an interest in meteorology, the same criterion of life that I apply to my own office. It is contained in the answer to the question, How do you show your interest in the advancement of our knowledge of the atmosphere? The reply that such and such volumes of data and mean values measure the contribution to the stock of knowledge leaves me rather cold and unimpressed.

But to return to the endeavor after the delicate adjustment between speculation and routine, which will reduce the waste of such an institution to a minimum; experience very soon teaches certain rules.

I have said elsewhere that the peculiarity of meteorological work is that an investigator is always dependent upon other people's observations; his own are only applicable in so far as they are compared with those of others. Up to the present time, I have never known any one take up an investigation that involved a reference to accumulated data, without his being hampered and harassed by uncertainties that might have been resolved if they had been taken in time. I shall give you an example presently, but, in the meantime, experience of that kind is so universal that it has now become with us a primary rule that any data collected shall forthwith be critically examined and so far dealt with as to make sure that they are available for scientific purposes—that is, for the purposes of comparison. A second rule is that as public evidence of the completion of this most im-

portant task there shall be at least a line of summary in a published report, or a point on a published map, as a primary representation of the results. Such publication is not to be regarded as the ultimate application of the observations, but it is evidence that the observations are there, and are ready for use.

You will find, if you inquire, that at the office we have been gradually lining up these troops of meteorological data into due order, with all their buttons on, until, from the commencement of this year, any one who wishes to do so can hold a general review of the whole meteorological army, in printed order—first order stations, second order stations, rainfall stations, sunshine and wind stations, sea temperatures and other marine observations—on his own study table, within six months of the date of the observations, upon paying to his Majesty's Stationery Office the modest sum of four shillings and sixpence. For all the publications except one the interval between observation and publication is only six weeks, and as that one has overtaken four years of arrears within the last four years, I trust that by the end of this year six weeks will be the full measure of the interval between observation and publication in all departments. This satisfactory state of affairs you owe to the indefatigable care and skill of Captain Hepworth, Mr. Lempfert and Mr. R. H. Curtis, and the members of the staff of the office who work under their superintendence. I need say little about corresponding work in connection with the Daily Weather Report, in which Mr. Brodie is my chief assistant, although it has received and is receiving a great deal of attention. The promptitude with which the daily work is dealt with hardly needs remark from me, though I know the difficulties of it as well as any one. If I spend only one long sentence in mentioning that on July 1, 1908, the morning hour of ob-



servation at twenty-seven out of the full number of twenty-nine stations in the British Isles was changed from 8 A.M. to 7 A.M., and the corresponding postoffices, as well as the Meteorological Office, opened at 7:15 A.M. in order to deal with them, so that we may have a strictly synchronous international system for western and central Europe, and thus realize the aspiration of many years, you will not misunderstand me to mean that I estimate the task as an easy one.

The third general rule is that the effectiveness of the data of all kinds, thus collected and ordered, should be tested by the prosecution of some inquiry which makes use of them in summary or in detail. It is here that the stimulating force of speculative inquiry comes in; and it is in the selection and prosecution of these inquiries, which test not only the adequacy and effectiveness of the data collected but also the efficiency of the office as contributing to the advance of knowledge, that the most serious responsibility falls upon the administrators of parliamentary funds.

Scientific Shylocks are not the least exacting of the tribe, and there have been times when I have thought I caught the rumination:

*Shy.* Three thousand ducats? 'tis a good round sum!

*Bas.* For the which, as I told you, Antonio shall be bound.

. . . . .

*Shy.* Antonio is a good man?

*Bas.* Have you heard any imputation to the contrary?

*Shy.* Oh! no, no, no, no. . . . Yet his means are in supposition: he hath an argosy bound to Tripolis, another to the Indies; I understand, moreover, upon the Rialto, that he hath a third in Mexico, a fourth for England, and other ventures he hath squandered abroad. But ships are but boards, sailors but men. There is the peril of water, winds and rocks. . . . Three thousand ducats.

We at the Meteorological Office are very much in Antonio's position. Our means of research are very much in supposition: four observatories and over four hundred stations of one sort or another in the British Isles; an elaborate installation of wind-measuring apparatus at Holyhead; besides other ventures squandered abroad; an anemometer at Gibraltar, another at St. Helena; a sunshine recorder at the Falkland Isles, half a dozen sets of instruments in British New Guinea, and a couple of hundred on the wide sea. The efforts seem so disconnected that the rumination about the ducats is not unnatural.

And you must remember that we lack an inestimable advantage that belongs to a physical laboratory or a school of mathematics, where the question of the equivalent number of ducats does not arise in quite the same way. The relative disadvantage that I speak of is that in an office the allowance for the use of time and material in practise and training disappears. All the world seems to agree that time or money spent on teaching or learning is well spent. In the course of twenty years' experience at a physical laboratory, and in examinations not a few, I have seen **M** and **N** or the wave-length of sodium light determined in ways that would earn very few ducats on the principle of payment by results; but, having regard to the psychological effect upon the culprit or the examiner, the question of ducats never came in. Wisely or unwisely public opinion has been educated to regard the psychological effect as of infinite value compared with the immediate result obtained. But in an office the marks that an observer or computer gets for showing that he "knew how to do it," when he did not succeed in doing it, do not count towards a "first class," and we have to abide by what we do; we can not rely on what we might have done. Consequently our means in supposition, spread over sea

and land, are matters of real solicitude. In such circumstances there might be reason for despondency if one were dependent merely upon one's own ventures and the results achieved thereby. But when one has the advantage of the gradual development of investigations of long standing, it is possible to maintain a show of cheerfulness. When Shylock demands his pound of flesh in the form of an annual report, it is not at all uncommon to find that some argosy that started on its voyage long ago "hath richly come to harbor suddenly." There have been quite a number of such happy arrivals within the last few years.

I will refer quite briefly to the interesting relations between the yield of barley and cool summers, or the yield of wheat and dry autumns, and the antecedent yield of eleven years before, which fell out of the body of statistics collected in the "Weekly Weather Report" since 1878. The accomplished statisticians of the Board of Agriculture have made this work the starting-point for a general investigation of the relation between the weather and the crops, which can not fail to have important practical bearings.

Let me take another example. For more than a full generation meteorological work has been hampered by the want of a definite understanding as to the real meaning in velocity, or force, of the various points of the scale of wind estimates laid down in 1805 by Admiral Beaufort for use at sea, and still handed on as an oral tradition. The prolonged inquiry, which goes back really to the report upon the Beckley anemograph already referred to, issued quite unexpectedly in the simple result that the curve

$$p = .0105B^3$$

(where  $p$  is the force in pounds per square foot, and  $B$  the arbitrary Beaufort number) runs practically through nine out of the eleven points on a diagram represent-

ing the empirical results of a very elaborate investigation. The empirical determinations upon which it is based are certainly not of the highest order of accuracy; they rely upon two separate investigations besides the statistical comparison, viz., the constant of an anemometer and the relation of wind velocity to wind pressure, but no subsequent adjustment of these determinations is at all likely to be outside the limits of an error of an estimate of wind force; and the equation can be used, quite reasonably, as a substitute for the original specification of the Beaufort scale, a specification that has vanished with the passing of ships of the type by which it was defined. This result, combined with the equation  $p = .003V^2$ , which has been in use in the office for many years, and has recently been confirmed as sufficiently accurate for all practical purposes by Dr. Stanton at the National Physical Laboratory and Monsieur Eiffel at the Eiffel Tower, places us upon a new plane with regard to the whole subject of wind measurement and wind estimation.

Results equally remarkable appear in other lines of investigation. Let me take the relation of observed wind velocity to barometric gradient. You may be aware that in actual experience the observed direction of the wind is more or less along the isobars, with the low pressure on the left of the moving air in the northern hemisphere; and that crowded isobars mean strong winds. Investigations upon this matter go back to the earliest days of the office.

There can be no doubt that the relation, vague as it sometimes appears to be upon a weather chart, is attributable to the effect of the earth's rotation. In order to bring the observed wind velocity into numerical relation with the pressure gradient Guldberg and Mohn assumed a coefficient of surface "friction," interfering with the steady

motion. The introduction of this new quantity, not otherwise determinable, left us in doubt as to how far the relation between wind and pressure distribution, deducible from the assumption of steady motion, could be regarded as a really effective hypothesis for meteorological purposes.

Recent investigations in the office of the kinematics of the air in traveling storms, carried out with Mr. Lempfert's assistance, have shown that, so far as one can speak of the velocity of wind at all—that is to say, disregarding the transient variations of velocity of short period and dealing with the average hourly velocity, the velocity of the wind in all ordinary circumstances is effectively steady in regard to the accelerating forces to which it is subject. This view is supported by two conclusions which Mr. Gold has formulated in the course of considering the observations of wind velocity in the upper air, obtained in recent investigations with kites. The first conclusion is that the actual velocity of wind in the upper air agrees with the velocity calculated from the pressure distribution to a degree of accuracy which is remarkable, considering the uncertainties of both measurements; and the second conclusion affords a simple, and I believe practically new, explanation upon a dynamical basis of the marked difference between the observed winds in the central portions of cyclones and anti-cyclones respectively, by showing that, on the hypothesis of steady motion, the difference of sign of the effective acceleration, due to curvature of path and to the earth's rotation respectively, leads to quite a small velocity and small gradient as the limiting values of those quantities near anti-cyclonic centers.

This conclusion is so obviously borne out by the facts that we are now practically in a position to go forward with the considerable simplification which results from regarding the steady state of motion in which

pressure gradient is balanced by the effective acceleration due to the rotation of the earth and the curvature of the path, as the normal or ordinary state of the atmosphere.

I can not forbear to add one more instance of an argosy which has richly come to harbor so lately as this summer. You may be aware that Kelvin was of opinion that the method of harmonic analysis was likely to prove a very powerful engine for dealing with the complexities of meteorological phenomena, as it has, in fact, dealt with those of tides. In this view Sir Richard Strachey and the Meteorological Council concurred, and an harmonic analyzer was installed in the office in 1879, but subsequently numerical calculation was used instead. A considerable amount of labor has been spent over the computation of Fourier coefficients. Not many great generalizations have flowed from this method up to the present time. I have no doubt that there is much to be done in the way of classifying temperature conditions, for climatic purposes, by the analysis of the seasonal variations. A beginning was made in a paper which was brought to the notice of the association at Glasgow. The most striking result of the Fourier analysis we owe to Hann, who has shown that, if we confine our attention to the second Fourier coefficient of the diurnal variation of pressure—that is, to the component of twelve-hour period—we get a variation very marked in inter-tropical regions, and gradually diminishing poleward in both hemispheres, but synchronous in phase throughout the 360 degrees of a meridian. The maximum occurs along all meridians in turn about 10 A.M. and 10 P.M. local time. This semi-diurnal variation with its regular recurrence is well known to mariners, and we have recently detected it, true to its proper phase, in the observations at the winter quarters of the *Discovery*; small in amplitude indeed—about a thousandth of

an inch of mercury—but certainly identifiable.

The reality of this variation of pressure, common to the whole earth, can not be doubted, and, so far as it goes, we may represent it (if indeed we may represent pressure differences as differences in vertical heights of atmosphere) as the deformation of a spherical atmosphere into an ellipsoid, with its longest axis in the equator pointing permanently  $30^\circ$  to the west of the sun. Its shortest axis would also be in the equator, and its middle axis would be along the polar axis of the earth. Somehow or other this protuberance remains fixed in direction with regard to the sun, while the solid earth revolves beneath it. Whatever may be the cause of this effect, obviously cosmical, and attributable to the sun, at which it indirectly points, its existence has long been recognized, and further investigation only confirms the generalization. It is now accepted as one of the fundamental general facts of meteorology.

Professor Schuster, for whose absence from this meeting I may venture to express a regret which will be unanimous, has already contributed a paper to the Royal Society pointing out the possible relations between the diurnal variations of pressure and those of terrestrial magnetic force. Going back again to the ubiquity of the application of the relation of pressure and wind, in accordance with the dynamical explanation of Buys Ballot's law, we should expect the effect of a pressure variation that has its counterpart in that of terrestrial magnetism to be traceable also in wind observation.

Mr. J. S. Dines has just given me particulars of the discovery of that effect in the great air-current, the variations of which I have called the pulse of the atmospheric circulation—I mean the southeast trade wind, the most persistent atmospheric

current in the world. It is difficult as a rule to get observers to pay much attention to that current, because it is so steady; but in 1891 the Meteorological Council set up an anemometer at St. Helena, in the very heart of the current, and we have just got out the results of the hourly tabulations. When the observations for the hours 1 to 24 are grouped separately for months, so as to give the vector resultants for each hour and for each month, it appears that there is a conspicuous semi-diurnal variation in the current, which shows itself as a closed polygon of vector variations from the mean of the day.

The month of April gives the most striking diagram of the twelve. It displays the superposition of two practically complete dodekagons, one a large one, completing its cycle from 6 A.M. to 6 P.M., the other a small one, for 6 P.M. to 6 A.M. The resultant wind for the whole day is very nearly southeast, and practically remains so for all the months of the year, the monthly variation of resultant wind being confined to a change of velocity from about fourteen miles per hour in May to about twenty-one miles per hour in September.

If, instead of combining the south and east components to form a vector diagram, we plot their variations separately, the semi-diurnal variation in each is plainly marked; and the calculation of its constants shows that its amplitude is about three quarters of a mile per hour both in the south and rather less in the east component. The easterly increment has its maxima at 10 A.M. and 10 P.M., and at these hours the phase of the variation of the southerly component is nearly opposite. Thus, to correspond with the semi-diurnal variation of pressure, there is a semi-diurnal variation in the trade wind at St. Helena, which is equivalent to the superposition upon the resultant wind of a northeasterly component of about one mile

per hour amplitude, with maxima at 10 A.M. and 10 P.M., the hours when the ellipsoidal deformation of the spherical atmosphere is passing over the locality.

I have only dealt with one month. I believe that when all the results that flow from this simple statement can be put before you, you will agree with me that the argosy which the Meteorological Council sent out in 1891 has indeed richly come to harbor.

Let me digress to say a word in illustration of the principle I laid down that, if one would avoid waste in meteorological work, the observations must be examined forthwith and so far discussed that any ambiguities may be cleared up.

After some years of wear at St. Helena the persistent rubbing of the southeast part of the spiral metallic pencil upon the metallic paper wore away the metal and left a flat place. This got so bad that the instrument had to come home for repairs, and when it was set up again, after a year's absence, the average direction of the trade wind differed by two points from the averages of most, but not of all, of the previous years. So far as we know, the orientation has been attended to, as before, and yet it is hardly possible to resist the suggestion that the anemometer has been set slightly differently. We are now making very careful inquiries from the observer; but, in the meantime, it seems to me that there is a great opportunity for a competent mathematical physicist to help us. Dynamical explanations of the trade winds have been given from the time of Halley. Let me offer as a simple question in the mathematical physics of the atmosphere whether a variation of two points in the direction of the southeast trade wind between the years 1903 and 1905 can be regarded as real, and, if not, which of the two recorded directions is the correct one?

It would be appropriate for me to add

some words about the results of last year's work upon the upper air, in which we have had the valuable cooperation of the University of Manchester. These results have disclosed a number of points of unusual interest. But we are to have an opportunity of considering that subject in a discussion before the section, and I need not deal with it here. I must, however, pause to give expression of the thanks of all meteorologists to Professor Schuster for his support of the Manchester University station at Glossop Moor. I may remind you that this generous contribution for the advancement of science on the part of Professor Schuster is in addition to the foundation of a readership in mathematical physics at Manchester and a readership in dynamical meteorology, now held by Mr. Gold at Cambridge.

I have said enough to show that the speculative ventures of official meteorologists are not all failures, and I will only add that if any mathematician or physicist would like to take his luck on a meteorological argosy he will be heartily welcomed. Part of the work will be drudgery; he must be prepared to face that; but the prospects of reaching port are reasonably good, so much so, indeed, that such a voyage might fairly lead to a claim for one of the higher academical degrees.

Up to now I have been dealing with the adjustment of official scientific work to reduce waste to a minimum, in so far as it lies within the control of those responsible for an office. I turn now to an aspect of the matter in which we require the assistance of others, particularly of the British Association.

The most serious danger of waste in a busy office is that it should carry on its work without an adequate knowledge of what is being done in advancing science and improving methods elsewhere. I speak myself for the Meteorological Office

alone, but I believe that the responsible officials of any scientific government department will agree with what I say.

Year by year some Timotheus "with his sounding flute and tuneful lyre" performs some miracle by the application of reasoning to the phenomena of nature. Only last year you heard Professor Love in his presidential address treat of the mundane question of the shape of the earth and etherealize the grim actualities with the magic of his spherical harmonics. Year by year, in every one of the subjects in which the practical world is immediately interested, active students, whether public officials, academic officials, or private enthusiasts, not only keep alight the sacred flame but occasionally add to its brilliance; and all the new knowledge, from whence-soever it comes, ought to be applied to the service of the state.

The actual volume of original contributions on these subjects is by no means inconsiderable. You are all aware that, some years ago, the Royal Society initiated a great international enterprise for the compilation of a catalogue of scientific literature. I have been looking at the fifth annual issue of the volume on "Meteorology," including "Terrestrial Magnetism." I may remark that the catalogue is quite incomprehensibly eclectic as regards official literature, but let that pass. I find that, in the year that closed with July, 1907, 1,042 authors (not counting officers and institutions as such) presented to the world 2,131 papers on meteorology, 229 on atmospheric electricity, and 180 on terrestrial magnetism. This will give some idea of the annual growth in these subjects, and may convince you that, after all allowance is made for duplicate titles, for papers of no importance, and for mere sheets of figures published for purposes of reference, there remains a bulk of litera-

ture too large for any single individual to cope with if he has anything else to do.

If instead of confining ourselves to what can be included in meteorology alone we extend our view over the other allied sciences, it would be necessary to take in other volumes of the international catalogue, and there would be some overlapping. I have taken instead the volume of the *Fortschritte der Physik* for 1906, which deals with "Kosmische Physik." It is edited by Professor Assmann, who adds to his distinction as head of the Royal Prussian Aeronautical Observatory of Lindenberg that of an accomplished bibliographer. In this volume are given abstracts or titles of the papers published during the year which can be regarded as worthy of the attention of a physicist. An examination of the volume gives the following numbers of the papers in the different sections:

	Papers
Astrophysics .....	222
Meteorology .....	1,122
Atmospheric electricity .....	135
Geophysics:	
Geodetics .....	105
Seismology and volcanic phenomena	256
Terrestrial magnetism and aurora ..	108
Currents, tides and waves .....	46
Inland hydrography .....	117
Ice, glaciers and ice age .....	139
Other papers .....	126
	897
Total.....	2,376

I need hardly say that these 2,376 papers are not all English; in some of the sections few of them are in that language, and fewer still are British. If British students, official and unofficial, are to make the most of the operation of drawing the angels down, they need help and cooperation in dealing with this mass of literature, in winnowing the important from the unimportant, and in assimilating that which makes for the real progress of

the practical application of science. This is the more necessary for these subjects because there is no organized system of academic teaching, with its attendant system of text-books. In a subject which has many university teachers it might reasonably be supposed that any important contribution would find its way into the text-books, which are constantly revised for the use of students; and yet, in his presidential address to the Royal Society in the November of last year, Lord Rayleigh felt constrained to point out that, for the advance of science, although the main requirement is original work of a high standard, that alone is not sufficient. "The advances made must be secured, and this can hardly be unless they are appreciated by the scientific public." He adds that "the history of science shows that important original work is liable to be overlooked and is, perhaps, the more liable the higher the degree of originality. The names of T. Young, Mayer, Carnot, Waterston and B. Stewart will suggest themselves to the physicist, and in other branches, doubtless, similar lists might be made of workers whose labors remained neglected for a shorter or longer time."

If this is true of physics how deplorably true it is of meteorology. If I allow a liberal discount of over 50 per cent. from the numbers that I have given, and estimate the number of effective contributions to meteorology as recognized by the "International Catalogue" at a thousand, which agrees pretty well with that given by the *Fortschritte der Physik*, and if I were to ask round this room the number of these papers read by any one here present, I am afraid the result would be disheartening. Many of us have views as to the way in which the study of meteorology ought to be pursued, but the views are not always based on an exhaustive examination of the writings of meteorologists.

Few of us could give, I think, any reasonable idea of the way in which it is being pursued by the various institutions devoted to its application, and of the progress which is being secured therein. Meteorological papers are written by the hundred, and, whether they are important or unimportant, they often disregard what has been already written in the same or some other language, and are themselves in turn disregarded. I do not think I should be doing any injustice if I applied similar remarks to some of the other subjects included in the table which I have quoted. How many readers are there in this country for an author in terrestrial magnetism, atmospheric electricity, limnology or physical oceanography? But, if the papers are not read and assimilated, the advancement of science is not achieved, however original the researches may be.

By way of remedy for the neglect of important papers in physics Lord Rayleigh suggests that teachers of authority, who, from advancing years or from some other reason, find themselves unable to do much more work in the direction of making original contributions, should make a point of helping to spread the knowledge of the work done by others. But what of those subjects in which there are no recognized teachers? and in this country this is practically the case with the subjects which I have mentioned. It is true that many of them are made the occasion of international assemblies, at which delegates or representatives meet. But such international assemblies are of necessity devoted, for the most part, to the elaboration of the details of international organization, and not to the discussion of scientific achievements. The numbers attending are, equally of necessity, very restricted.

The want of opportunity for the discussion of progress in these sciences is spe-

cially lamentable, because in its absence they lose the valuable assistance of amateur workers, who might be an effective substitute for the students of an academic study. In no subject are there more volunteers, who take an active part in observing, than in meteorology; but how few of them carry their work beyond the stage of recording observations and taking means. The reason is not lightly to be assigned to their want of capacity to carry on an investigation, but far more, I believe, to the want of knowledge of the objects of investigation and of the means of pursuing them.

Among the agencies which in the past have fostered the knowledge of these subjects, and stimulated its pursuit, there stand out prominently the annual meetings of this association. It was the British Association which in 1842 re-founded the Kew Observatory for the study of the physics of the atmosphere, the earth, and the sun. It was the British Association which promoted the establishment of magnetic observatories in many parts of the earth, and in the early sixties secured the most brilliant achievements in the investigation of the atmosphere by means of balloons. I know of no other opportunity of anything like the same potentialities for the writers of papers to meet with the readers, and to confer together about the progress of the sciences in which they are interested. But its potentialities are not realized. Those of us who are most anxious for the spread of the application of mathematics and physics to the phenomena of astronomy, meteorology and geophysics have thought that this opportunity could not properly be utilized by crowding together all the papers that deal with such subjects into one day, or possibly two days, so that they can be polished off with the rapidity of an oriental execution. In fact, the opportunity to be

polished off is precisely not the opportunity that is wanted. There are some of us who think that a British Association week is not too long for the consideration of the subjects of which a year's abstracts occupy a volume of six hundred pages, and that, if we could extend the opportunity for the consideration of these questions from one or two days to a week, and let those members who are interested form a separate committee to develop and extend these subjects, the British Association, the country and science would all gain thereby. I venture from this place, in the name of the advancement of science, to make an appeal for the favorable consideration of this suggestion. It is not based upon the depreciation, but upon the highest appreciation of the service which mathematics and physics have rendered, and can still render, to the observational sciences, and upon the well-tried principle that close family ties are strengthened, and not weakened, by making allowance for natural development.

The plea seems to me so natural, and the alternatives so detrimental to the advancement of science in this country, that I can not believe the association will turn to it a deaf ear.

W. N. SHAW

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#### GRAPHIC ART IN SCIENCE<sup>1</sup>

As a beginning I wish to thank this society for the privilege granted me to address it. That a strong personal pleasure is felt at this opportunity, I shall not attempt to deny; but I have a greater satisfaction than this, and that is that I am allowed to present a subject which has been too seldom publicly discussed in the presence of investigators and students, viz.: the part played by graphic art in science.

This subject is one of growing importance, and I trust I shall live to see the day

<sup>1</sup>Read before the Harvey Club, University of California, November 7, 1907.